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EXPERIMENTAL PARAPSYCHOLOGY IN
BRITAIN: A SURVEY OF PUBLISHED
WORK, 1948-53¹

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I. ESP EXPERIMENTS WITH CARDS AND DRAWINGS

IN the winter of 1947, when Dr S. G. Soal delivered in London the Ninth Myers Memorial Lecture, there was some feeling that experimental work in England was not leading anywhere (21). Attempts to carry out ESP research at the headquarters of the Society for Psychical Research were yielding nothing of significance. I had myself just published the results of a long series of entirely negative card-calling tests (30), and an energetic group of Cambridge undergraduates had achieved no conclusive results in experiments designed to investigate the effect of varying combinations of subject and agent in telepathy tests (22). A little earlier in the year some mass ESP tests using drawings as targets had been published. These had had wide publicity, and hundreds of persons took part, but in spite of several repetitions, and in spite of most conscientious matchings and scorings of various kinds by Mr M. T. Hindson and other judges, no evidence of any extra-chance effects was found (29). It seemed that the only dependable ESP results came from exceptionally gifted subjects, like Mr Basil Shackleton and Mrs Gloria Stewart, but at that time no such persons were generally available and the outlook was gloomy.

Dr Soal appeared to give some support to the defeatists by saying in his lecture that he did not think much would be achieved by planned experiments with unselected persons. All too often mass experiments led to waste of time and labour, to totally inconclusive

¹ This article was first published in the *Journal of Parapsychology* for March 1954. It is printed here, by courtesy of the Editor, with minor modifications and additional material on British research published in the United States.

or totally negative results. Dr Soal was well aware that it was the policy of many American experimenters to apply ESP tests to groups of unselected persons, but he based his opinion on the disappointing experience of British investigators who had adopted that policy, and on the statistically unimpressive results of some of the American work. But Dr Soal was not content to sit back and deplore the situation; he asked the officials and members of the S.P.R. to make an all-out effort to find some first-class sensitives. He urged them to cast the net wider, for there must be as good fish in the sea as ever came out of it. Dr Soal's appeal had a good response, and some very interesting findings were reported in the years that followed. As so often happens in this subject, the findings were not what the instigators of the research anticipated.

Experiments with Mrs Stewart

There was, of course, one outstandingly productive piece of research in progress in 1947, namely Dr Soal's own experiments with the gifted subject Mrs Gloria Stewart. Her ability had been discovered in 1939, but it was not until after the war that she was in London and available for experiments. Dr Soal utilised his Myers Lecture to give a progress report on these experiments, but as he did not then give the full data, and his complete report is shortly to appear in book form,¹ it would be superfluous to attempt to describe the research here. There is one section of the work, however, that Dr Soal and his collaborator Mr F. Bateman have already published in full: the long-distance tests carried out in the summer of 1949, with Mrs Stewart in Antwerp, Belgium, and the target cards in London (1).

These distance tests were carried out under GESP conditions, with either Mrs Hales or Mrs Holding acting as agent. Mrs Stewart had previously worked successfully with both these ladies. The target cards were turned over at three-second intervals, and Mrs Stewart made her calls at the same rate, both operations being timed by stop-watch. Starting points were synchronised by means of the B.B.C. time signal. Mrs Stewart kept up the same high level of scoring that she was giving before she went away; the distance made not the slightest difference. The odds against the score she obtained in these distance tests arising by chance exceeded 500 million to one. The tests established two interesting points. Changing the location of the agent and the cards without telling Mrs Stewart did not affect her scoring. Introducing an extra agent, who looked at an opposing set of targets, did not

¹ S. G. Soal and F. Bateman, *Modern Experiments in Telepathy* (London, Faber; New Haven, Conn., Yale University Press).

affect her scoring on the primary agent's targets. Another series of distance tests, in which various persons in Cambridge acted as agents, all of them strangers to Mrs Stewart, yielded only chance results. This suggests that Mrs Stewart needs some personal contact with the agent before her ESP can function.

A Dispersion Effect?

During 1948 and 1949, as mentioned in the S.P.R. annual reports for those years, Mr J. Fraser Nicol and Mr Edward Osborn conducted a series of GESP tests with individual subjects. In most of these tests agent and subject were in separate rooms. The experimenter, who sat in a third room, had beside him a bank of five switch buttons connected with five lamps which illuminated the different target symbols that were displayed before the agent and simultaneously lit a signal light beside the subject. The total score produced by the subjects in these experiments was depressingly close to chance expectation (a deviation of 39.2 in 15,769 trials). However, one point of possible significance was discovered later. In 1950, I obtained some ESP results suggestive of a dispersion of subjects' scores, some going above chance expectation and some below. This was in a series of clairvoyance tests using closed packs of standard ESP cards shuffled by a collaborator and placed in small wooden boxes. The tests were really a fiasco. The intention was, during the course of the card tests, to administer to each subject a drug that was supposed to induce euphoria, and to observe any effect this might have on the level of scoring. I did not try out the drug properly in advance, and the only noticeable effect it had was to cause one of the subjects to go into an hysterical stupor. There were five subjects, each of whom completed 25 runs in a single session. The total result was uninteresting, but some of the subjects scored above chance and some below, and the dispersion of their scores was statistically significant ($P=0.01$).

It is never justifiable to base a conclusion on a single observation of an unexpected 'effect', since this could so easily be a fluke of chance. However, positive or negative scoring according to temperament or circumstance is a well-known ESP response. It has also been suggested that the ESP effect is often transitory and only to be seen at the commencement of the tests. In view of these two considerations, it seemed reasonable to examine, say, the first 16 runs of every subject in an ESP investigation and find out if these scores were significantly dispersed from the chance average. This was done with the Nicol-Osborn data, and a significant dispersion was found ($P=0.01$). It is a test that might be applied with profit to other collections of superficially uninteresting data (33).

Self-testing Experiments

To return to 1949, only one British experimenter published any ESP results that year. Dr R. H. Thouless sent to the *Journal of Parapsychology* an account of some self-testing trials under several different conditions. Now it is customary in ESP research to disregard unwitnessed trials because of the danger of recording errors when there is no second person to check the scoring. Considering the slight and unrepeatable nature of many ESP results, this seems to me wise, and I think Dr Thouless's contribution would have been more valuable if he had taken the conventional precautions. The tests in question were DT trials with a closed pack of standard ESP cards. The cards were hand-shuffled and then cut at a point determined by a random procedure. In one series the guesses were made after the shuffle and cut, in another they were made after the shuffle but before the cut, and in a third series they were made before both the shuffling and the cut. The first method yielded a significant negative deviation (odds 100 to 1), the second gave a chance result, and the third a positive deviation (odds 50 to 1) (24).

A Search for ESP Subjects

Apart from this work by Dr Thouless, no British ESP reports had appeared since the Myers Lecture, and none was expected in the immediate future. It certainly seemed, in 1949, that ESP research was in the doldrums. Dr Soal was the only investigator who had a good ESP subject at his disposal. I myself had produced no positive results since becoming Research Officer to the S.P.R. My appointment was terminated that year, and I journeyed to Duke University in the hope of discovering the secret of success in ESP experiments. I learned a great deal, but the secret was elusive. Back in London, however, an important development took place. After an absence of twenty years, Mr G. W. Fisk returned to active participation in research and undertook the heavy task of implementing Dr Soal's suggestion that an organized search should be made for some gifted ESP subjects.

The method was simple. As many persons as could be interested in the matter were given ESP cards, score sheets, and an instruction leaflet, and were asked to carry out tests on each other in their own homes, sending their completed score sheets to Mr Fisk. It was not thought that any conclusions could be based on these informal, unsupervised tests, but it was hoped that, if there were any good subjects among the persons taking part, their scores would be outstanding and they could be followed up by experimenters from the S.P.R. (4).

The search went on for over two years and Mr Fisk collected a large body of data from a total of 236 subjects. All those subjects whose initial scores were in the least promising were encouraged to continue, but in the final outcome only one gave a consistently significant score. This was Mr M. B., an electronic engineer. For once luck was on the side of the investigation. M. B. was a colleague of Mr A. M. J. Mitchell, who is an S.P.R. member and a competent experimenter. Mr Mitchell was present at the experiments, and he attended to the randomisation of the target cards, supervised the conditions of the tests, and checked the scores. On some occasions additional observers were present. Mr D. C., who was M. B.'s assistant, acted as agent in the tests. They were separated by a distance of 12 feet with a large cupboard intervening. When their roles were reversed and D. C. was made the subject the scores were insignificant. At the first four sessions of 100 trials each, M. B. averaged 12 calls correct per run—better than most of the famous high-scoring subjects. Mr Fisk attended the fifth session in the hope of witnessing the phenomenon, but the scores dropped to chance level and remained there in all subsequent tests. M. B. himself professed scepticism of ESP and said his results must be a fluke of chance, although the odds, more than 500,000 million to 1, were too big for that to be a reasonable explanation. Unless there was some undetected trickery or leakage, which seems unlikely, it was a case of spectacular but short-lived ESP ability, another of these episodes that serve to tantalise and mystify the investigator without bringing him any nearer to a solution of his research problems (13).

An Unexpected Result

The results from the other 235 subjects were not wasted. Early in the research Mr Fisk noticed a curious fact. Although the number of direct hits was close to chance expectation, there were significant displacement effects in the form of negative deviations on the cards one ahead and one behind the target. At first we were not much impressed, for it was an unlooked-for effect that could have been a fluke, or perhaps an artefact due to recording errors or faulty shuffling. But as more and more data came to hand, and the displacement deviations accumulated steadily, it became clear that at least they were not due to chance. The effects were small, but they were surprisingly persistent throughout the large mass of data. The deviations on the plus 1 and minus 1 displacement targets were about equal. There were no significant deviations on the target card itself or on the plus 2 and minus 2 positions. By way of control, a large-scale cross check was carried out by Mr

Fisk. He matched the subjects' calls against the target cards of the immediately succeeding run, but this gave only chance results, no displacement effects whatsoever. It seems unlikely, therefore, that faulty shuffling of the target cards or artefacts due to the use of closed decks could be the entire explanation of the results, for these factors would presumably also affect the cross check scores. Moreover, such analyses as were carried out on the data showed that the target sequences did in fact conform to statistical randomness in as many respects as were tested. Sensory leakage, and errors in recording targets and calls, may have occurred, but it is difficult to see how the observed results could have arisen out of such defects. If the experiments were so carelessly conducted that the subjects received occasional hints as to the order of the cards, one would expect them to get a positive score on the card at which they were aiming, but this did not happen. If the effect had been simply a negative deviation on the card one behind, this could have been accounted for by supposing that the subjects were able to tell from the experimenters' reactions when they called a card correctly, and that when this happened they tended to call a different card at the next trial. This would automatically produce a negative minus 1 displacement. In actual fact there was an equally deviant score on the card one ahead, which is more difficult to explain (35).

It is only fair to mention that these findings have been criticised in some personal communications. One investigator suggested that the slight negative scores might be due to miscounting, but the data have been checked too often for that to be possible. Another suggested that a combination of recording errors, sensory leakage, and statistical artefacts might account for the results. I do not see how this would work, but that does not mean to say it is impossible. One cannot be more definite because these experiments were never intended as crucial ESP tests. Nevertheless, they suggested an interesting hypothesis. If the displacement effects were really due to ESP, this would seem to imply that a large number of persons, working in different places and circumstances, contributed to the same odd and unexpected result. Could it be that the organizing experimenter, Mr Fisk, even though he was not present in person at the tests, could so influence the subjects taking part as to cause a substantial portion of them to respond all in the same peculiar manner?

A New Technique : Clock Cards

Before considering this possibility further, one other development must be described, namely the introduction of clock face cards. Once again Mr Fisk was the leading spirit. There had

been some discussion about the use of quantitatively varying targets, such as shapes and sizes, which would allow of the evaluation of approximate guesses (near misses) as well as direct hits. Mr Fisk invented an ingenious system. He designed a set of cards bearing a replica of a clock face with an hour hand pointing to one or other of the twelve hour positions. The subject had to guess which hour was indicated on the card. There were thus twelve possible calls instead of the usual five, but as clock faces are universally familiar the subjects could keep in mind all twelve possibilities without effort. Moreover, near misses could be taken into account. Mr Mitchell evolved a 'divergence scoring system' in which a maximum score of six points was given for a correct call, five for a call one hour out, and so on. Calls six hours away from the target got no credit.

Mr Fisk first tried out the method on his two grand-children, using randomised clock targets and informal GESP conditions. The two children competed with each other, but whereas one of them (S. B.) achieved a significant number of correct calls (odds over 10,000 to 1), the other always got chance scores. The tests went on intermittently for over a year, but in the later sessions S. B. was becoming bored and there was a distinct decline in her scoring level. When evaluated by the divergence scoring system, S. B.'s data yielded slightly larger odds (30,000 to 1) due to the fact that, in addition to her correct calls she also obtained a smaller, but significant, excess of calls that were only one hour out.

Mr Fisk next applied the new card tests to 17 other persons. He arranged the target cards according to random numbers and enclosed them, in sets of twelve, inside sealed envelopes. These he dispatched to the subjects with instructions to guess the order of the cards inside the envelopes going from top to bottom of the pack. Score sheets were provided for recording the calls, and these were to be returned to Mr Fisk, together with the envelopes still unopened. Mr Fisk used various methods of sealing which would have revealed any attempt to open the envelopes, but there was no evidence that any of the subjects tried to tamper with them. One subject, Miss S. M., produced scores so outstanding that it would be misleading to group her with the others. The remaining 16 subjects gave just about the chance average of correct calls, but on the divergence scoring system their data gave odds of over 1,000 to 1. Once again this was due to a significant excess of calls that were only one hour away from the target. Miss S. M., the outstanding subject, gave a great excess of correct calls. She had a deviation of plus 20 in 240 trials, which corresponds to odds of about 300,000 to 1 (5).

The Influence of the Experimenter on ESP Results

More research was urgently called for to find out whether these unusually good results were due to the merits of the clock cards, to the novelty of the technique, or to special qualities in the experimenter, Mr Fisk. In order to investigate the last question, Mr Fisk and I joined forces in a combined experiment. Sealed envelopes, each containing decks of twelve randomised clock cards, were sent to twenty subjects. Each subject received in all 32 envelopes, all of them enclosed in outer covers marked by Mr Fisk and posted from his home in Surrey. Unknown to the subjects, half of the envelopes contained cards randomised by me and never seen by Mr Fisk. These envelopes Mr Fisk returned to me unopened, together with the subjects' calls, so that I could score them independently. The result was that the subjects gave a significant excess of correct calls on the target cards prepared by Mr Fisk (odds over 6,000 to 1) and a chance score on those prepared by me. This appeared to confirm what had previously been suspected, namely that different experimenters obtain different results even when they work under the same conditions and with the same subjects. In this case the difference was what might have been anticipated: Mr Fisk proved to be the experimenter who elicits successful scores, whereas I seemed to act as an inhibitor. But there was another possibility. The subjects were allowed to assume that Mr Fisk prepared all the cards, so that their thoughts were presumably directed to him. If they were obtaining their correct calls by telepathy, this might account for their being unable to read my targets. Telepathy, however, seems an implausible explanation. Both experimenters prepared large batches of cards in advance and had no recollection of the order inside any particular envelope. When one considers the very slender contact the experimenter has with the subjects in this form of test, it is very mysterious and surprising that he should have an influence on their results.

There was one instructive exception to the general rule about the difference between the results of the two experimenters. Without telling me, Mr Fisk included the outstanding subject Miss S.M. among those tested. She gave significantly positive results with both of us, with odds of about a million to one for my section of her data. Whatever may be my powers as a psychic inhibitor, they were insufficient to quell this gifted lady (36).

The Search for Personality Correlates

Another of the suggestions made by Dr Soal in his Myers Lecture was that experimenters should follow up the promising

researches of Drs Humphrey and Schmeidler, and investigate the connection between the personality attributes of subjects and the nature of their ESP responses. Mr W. G. Roll at Oxford is now engaged on this, but otherwise very little work of the kind has been done in England. The reason is fairly obvious. This type of research demands of the experimenter some training in the application of personality tests and an ability to handle statistical problems more complex than those encountered in most ESP research. In addition, he must have plenty of time to collect material, because the differences between the scoring levels of the various personality groupings are usually so marginal that they can only be demonstrated in a large body of data. In practice it is virtually impossible for anyone except a more or less full-time laboratory worker to carry through these investigations satisfactorily. After visiting the Duke Parapsychology Laboratory, and becoming very enthusiastic about the personality approach to ESP, I made one determined attempt to apply a technique devised by Dr Betty Humphrey. Subjects were made to do freehand drawings and their products were rated expansive or compressive according to Elkinsch's criteria. The collection and analysis of the data from fifty subjects involved months of work, but in the outcome the ESP tests were inconclusive and showed no definite correlation with the expansive-compressive ratings. The experiments did convince me that the Elkinsch criteria measure real differences between people, but they contributed nothing to knowledge of ESP (32).

Experiments with Mental Patients

Related to the research into the personality correlates of ESP is the investigation of psychic abilities in the abnormal. Some informal tests, instigated by the well-known psychiatrist Professor Urban of Innsbruck, suggested that mental patients made particularly good ESP subjects, especially just after undergoing electroshock therapy. In 1950, during a visit to England, Dr Betty Humphrey, assisted by Miss Elizabeth McMahan, conducted a series of ESP tests on mental patients in two London hospitals. They failed to discover any significant difference between the results before and after shock treatment; in fact the only section of the data which suggested the presence of ESP was that obtained from a small group of schizophrenic patients (8).

It is in accord with the theories of Dr Jan Ehrenwald that schizophrenic patients should manifest exceptional ESP capacity. In 1951 I gave ESP card tests to a group of psychotic patients, chiefly schizophrenics, but obtained only chance results. Many of the patients were suspicious and unco-operative, and the work was

arduous and unrewarding. In my view there is no clear evidence that mental patients make specially good ESP subjects, although a sufficiently large collection of data might reveal consistent differences between the ESP responses of the various diagnostic groups. It might also be profitable to explore the relation between the extreme moods and attitudes shown by some mental patients and their performance in ESP tests. One of the groups I tested consisted entirely of antagonistic, distrustful patients. It was thought that perhaps these would give negative scores, but in the event their results were insignificant (34).

A Psycho-analytical Approach

In 1952 Mr F. Claude Palmer was awarded a Master's degree at London University on the basis of a research involving a psycho-analytical approach to telepathy (16). He tested one subject, a lady artist, who believed she possessed special psychic ability. At each trial, which lasted ten minutes, an agent sat looking at a coloured reproduction of a painting. He also wrote down the very personal associations that the picture called to his mind. Meantime, the subject sketched and painted her impressions. The purpose of the tests was to determine whether the subject's ESP impressions were particularly successful in connection with those of the agent's associations that had special emotional significance judged from the psycho-analytic point of view. The work was intended as an exploratory venture, the results have not been published in detail, and it appears that the statistical evaluation was not entirely satisfactory. However, Mr Palmer concluded that the subject was unsuccessful at reproducing the formal content of the pictures, but successful at detecting the agent's personal associations.

Electric Shocks for Errors

Only one more card-calling research remains to be described, namely the tests conducted in 1950 at the Department of Psychology of Glasgow University by Mr W. A. McElroy and Miss Winifred R. K. Brown (12). The particular feature of the tests was that in half the runs the subjects received an electric shock whenever they guessed wrongly. The purpose was to investigate the possibility that punishment might stimulate learning to avoid mistakes. The subjects guessed the colours of playing cards—red or black. The method of target selection was hardly satisfactory. Since a closed pack was used the card had to be replaced and the pack re-shuffled after every call. Moreover, the experimenters, who each worked alone, wrote down both call and target, so that there was no check on the accuracy of the recording. Considering

that ESP tests have been commonplace for more than twenty years, one would have thought that by now the standard techniques (including target randomisation and independent recording of target and call) would be firmly established; yet these Glasgow experimenters are not alone in failing to use the proper methods of testing. When evaluating the results of such imperfect experiments one has always to keep in mind the reservation 'subject to further confirmation'. As ESP researches are usually either not repeated or unrepeatable, this is almost tantamount to writing off the results as a dead loss. In the case of the Glasgow experiments, the total trials gave a significant positive deviation, odds 100 to 1, but no evidence of improved scoring with practice. On the contrary, the usual chronological decline was noted. The tests with electric shock gave a bigger deviation than those without, but the difference was not statistically significant.

Unpublished Work

In addition to the work described in this review, projects have from time to time been organized by groups up and down the country, including about half a dozen associated with universities. Their activities are little known for they seldom write reports for publication. Sometimes they feel the results do not warrant it; sometimes they are aware that their experimental methods do not conform to the standards of most published work. On the latter point, the S.P.R. has available a pamphlet giving advice on methods of testing that are both adequate and easy to apply (36). On the question of negative results, experimenters should realize that other workers need to know what has been done irrespective of the statistical outcome.

II. EXPERIMENTS WITH MEDIUMS

Object-reading

Another approach, recommended by Dr Soal in his Myers Lecture as offering real scope for experimental work, was the investigation of object-reading, sometimes called psychometry. There are two fundamental difficulties in most work with mediums. The first is that it is impossible to set any precise limits to the impressions and deductions the medium might obtain from the sitter's appearance and reactions. The condition of silence, and the separation of agent and subject by walls or screens, which is standard practice in experimental tests of telepathy, would not be tolerated by most mediums. But if the medium does not demand the presence of a sitter, and can establish the necessary link by

merely handling nondescript articles—which may even be enclosed in boxes or envelopes—then the difficulty is at once overcome. In object-reading the medium's impressions are usually supposed to relate to the personal circumstances of the absent owner of the article, and the reading is sent to him for annotation. The second difficulty in research with mediums is that there is no objective measure of the accuracy of the medium's impressions; the investigator is dependent upon the judgement and testimony of the sitter, which may be both fallible and biased. Under the conditions of object-reading tests for an absent owner, this second difficulty can also be overcome. The owner of the article is given several different readings from the same medium and is asked to select the one that fits him best. If he picks on the one that was intended to relate to him, this is independent evidence of the appropriateness of the medium's statements.

Even supposing that the medium's impressions are only vaguely appropriate, this should be sufficient at least to enable the absent owners to identify their readings. However, in a long series of experiments, the results of which were published in 1949, I found no evidence that this happened in actual practice (31). In this series, 12 mediums, some of them quite well known, gave a total of 97 readings. The absent owners were sent several readings at a time and asked to select the one most appropriate to them. They selected their own reading no more often than would be expected by chance.

It is conceivable that when a reading has to be accepted or rejected as a whole, some interesting parts may escape notice. An attempt was therefore made to devise a more sensitive method for detecting a slight, intermittent paranormal content in the readings. Mr M. T. Hindson prepared an enormous catalogue of mediums' statements classified according to topic and specificity. A series of 15 readings was divided into sections dealing with independent topics. Each section was paired with a control section made up from the catalogue and consisting of the same number of statements, of about the same specificity and on the same topic as those in the section taken from the actual reading. Instead of whole readings and a single choice, the owner was given a series of sections of a reading, each paired with a control, and he was asked to select out of each pair the section he found the more appropriate. This method also gave only chance results.

The readings given by the mediums in my tests were unimpressive even on casual inspection, being far too vague and imprecise. If the same tests had been tried with one of the famous mediums of the past, Mrs Piper for example, the results might have been

different. The only reports of successful results from systematic object-reading tests with contemporary mediums come from Dr J. Hettinger. I have already criticised some of his earlier work (28), which suffered from various defects of method. For instance, Dr Hettinger, who knew who the owners of the objects were, was present while the medium was giving her impressions, and he was also the final judge of doubtful annotations. He also asked the annotators to mark the readings statement by statement, and treated the scores so derived as if all the statements were independent of each other. In 1949 Christopher Scott published a long analysis of Dr Hettinger's experiments showing that no reliance could be placed on his results (20).

All that is an old story, but more recently Dr Hettinger developed a new method in which the medium was in effect used as a telepathic subject. While she was giving her object-reading impressions, the owner of the article looked at magazine pictures, and the medium's statements were afterwards compared with the content of these pictures. Dr Hettinger reported some very striking coincidences, which he felt must be due to ESP. He went on to make deductions about 'time lag' and 'psychic distortion', all of which were based on the coincidences he had noted. His experiments attracted some publicity in England, and in September 1947 he read a paper about them before the British Association for the Advancement of Science. The great criticism of all this work was that Dr Hettinger completely neglected the factor of chance. In his many thousands of comparisons of statements and pictures, some striking coincidences would be expected, indeed these might account for all his observations. Some attempt to assess the chance factor was made in an experiment with the American S.P.R., in which the medium's statements were matched against a series of control pictures as well as against the pictures actually looked at during the experiments. The results were quite inconclusive (6). In 1948 Christopher Scott conducted a test with Dr Hettinger in which he substituted control pictures for some of those actually contemplated during the experiment. Dr Hettinger matched the medium's statements against the pictures in the usual way, not knowing that some of them were controls. He thought the results were good and up to the standard of his other experiments, and he found as many resemblances as usual. In point of fact the control pictures contributed just as much as the originals to the resemblances Dr Hettinger pointed out. It was as neat a demonstration as could be desired that his subjective evaluation, upon which all the previous results depended, was simply unacceptable.

Dr Hettinger thought, wrongly I believe, that a spirit of animosity pervaded the criticism of his work (7). It is a necessary evil, sometimes, to devote energy to exposing defects in a colleague's experiments. In our subject, injudicious claims or manifestly faulty methods, unless smartly dealt with, would soon bring the whole field into disrepute. Object-reading tests provide great scope for experiments, but they need as rigid precautions as ESP tests if the results are not to be influenced by biased annotation or sensory leakage. The points made by Scott and myself in this connection have been more succinctly put by Pratt and Birge in their excellent paper in the *Journal of Parapsychology* (17). These workers also introduced a method whereby scores from itemised readings could be evaluated statistically notwithstanding the interdependence of the items. A cursory examination of the readings from my tests, which had also been annotated item by item, was sufficient to show that the scores were too close to chance expectation for the Pratt-Birge method to give any significant result. At the present time, Mrs Kathleen Gay, a member of the S.P.R. Council, is conducting more object-reading tests, and it may be that her results will prove more interesting.

One cannot leave the topic of object-reading tests without mentioning the precognition experiment carried out in 1949 by Mrs V. M. Austin. The method was suggested and reported by the late G. N. M. Tyrrell (25). Mrs Austin gave a medium a small article to handle and asked for impressions about its associations in the future. Two days later she put the names of thirty acquaintances on slips of paper, mixed the papers in a hat, and picked one out haphazardly. She sent the article to the lady thus selected, who carried it about for a year. At the end of that time the lady was shown the medium's statements. The statements were also seen by the other twenty-nine persons, whose annotations were intended to act as a control. It appeared that the medium's statements were more appropriate to the lady who actually carried the article than to any of the other persons. Unfortunately the system of annotation was very unsatisfactory and no claims were made for the apparently positive results. However, the method of adapting object-reading to a test for precognition is worth remembering.

Experiments with the Electro-encephalograph

A novel form of experiment was carried out in 1951 by Mr Edward Osborn and the electro-encephalographer Dr C. C. Evans with the medium Mrs Eileen Garrett. Using an eight-channel electro-encephalograph with a frequency analyser, they took recordings of the brain rhythms of Mrs Garrett when in her normal

state, when entranced and 'controlled' by the personalities known as 'Uvani' and 'Abdul Latif', and also after being hypnotised by the psychiatrist Dr E. B. Strauss. No significant differences were found between the recordings under these three conditions, but it would be worth while to apply the same test to a series of mediums (3).

In 1951 Dr S. C. Wallwork of Nottingham University took electro-encephalographic recordings from a subject while he was engaged in ESP tests. The subject spoke his calls softly, from a reclining position so as not to interfere with the recordings. The aim of the experiment was to determine whether there was any relation between correct ESP calls and variations in the alpha rhythm of the brain. No correlation was discovered, but it must be noted that the ESP scores during the recordings were insignificant, although the subject had previously given significant scores. Clearly more investigation is needed (27).

III. PK RESEARCH

In the matter of PK experimentation, there was a long gap between the appearance of the first American reports (in 1943) and the publication of any substantial positive results in British experiments. This is reminiscent of the situation in the early years of ESP research at Duke, when British investigators tried in vain to reproduce the American results. In 1945 Mr Denys Parsons published the results of some unfruitful PK tests in which dice were thrown inside a rotating cage, and Mr Dennis Hyde reported a series of dice throws that were completely uninteresting save for some spurious position effects due to dice bias. In the same year Dr Thouless reported some slight positive results in coin-spinning tests, but that was all. Further dice tests at S.P.R. headquarters (unpublished) yielded nothing.

A distinctly sceptical attitude became prevalent in London. It was pointed out by Dr Thouless and others that some of the American PK experiments were not well designed. In particular, there were more throws for sixes than for other faces, and sometimes there were changes of dice, which made it difficult to distinguish a genuine PK effect from the results of dice bias. Moreover, some of the better-designed experiments showed no direct correlation between the subject's intention and the outcome of the throws, and in these cases position effects provided the only evidence for the presence of an extra-chance factor. In the Myers Lecture Dr Soal pointed out that the behaviour of dice is likely to be erratic and non-random. The probability of a given

face appearing uppermost varies from one throw to another according to the initial position of the die, the initial angular velocity, the nature of the surface on which it falls, and its changing elasticity factors. These variables might produce spurious position effects if the expected face frequencies changed systematically according to the number of throws. Mr Denys Parsons went into the question in some detail, and his views lent force to the general attitude of scepticism, although up to the present no detailed critique has been published.

Nicol-Carington Experiments

In 1947 Mr Fraser Nicol and the late Whately Carington published the results of several long series of die throws, most of them quite null. The main series consisted of 115,200 witnessed cup throws of the same die by eight subjects, 14,400 throws by each subject and an equal number of throws for each face. The total score gave no evidence that the subjects succeeded in throwing the faces they desired, but a secondary effect was noted. The scores on the high targets were significantly positive, but in the total this was masked by a significantly below chance score on the low targets. It cannot be said that this was a convincing proof of PK, especially considering the smallness of the effect (15).

Dr Knowles's Experiments

It was two years before another report of positive results in dice throwing appeared, and this time it came from an unexpected quarter (9). Dr E. A. G. Knowles, an engineering mathematician, desiring to illustrate one of her lectures on statistics with a practical demonstration, asked members of her audience to take it in turn to throw a set of twelve dice, aiming alternately for high faces and low faces. She expected to be able to demonstrate a random frequency distribution of the faces which would be unaffected by the subjects' aims. In the event, there was a significant difference in the proportions of high faces and low faces according to the intention of the throwers, the odds amounting to 240 to 1. Dr Knowles explored the matter further by loading a die with lead at the corner of the 4, 5, and 6 faces, so that it was biased to favour low faces. Actual trials showed that the bias was just about the same as that produced by the subjects in the willing experiment. The PK effect, if such it was that she had observed, was equivalent to the effect of a very palpable set of physical forces acting on the dice.

In 1952 Dr Knowles reported a second PK experiment (10). In this she used a pointer, spun by hand, and brought to rest by a manually operated break pad. The subjects' task was to

apply the break at such a moment that the pointer, which was hidden from view, stopped over a specified one of 15 marked sectors. Since the spun pointer was not an autonomous mechanical system, the procedure was called a test of psi dexterity rather than a test of P K. There was a statistically significant excess of hits on target sectors, as well as an excess on sectors adjacent to the target. Rotation of targets precluded an explanation in terms of apparatus bias.

Dr Thouless's Experiments

The next British P K report was another from Dr R. H. Thouless (23). His main work comprised two series in which four dice were mechanically released so as to strike a corrugated surface. In the first series the targets were decided by 6×6 Latin Squares. This ensured that there would be an equal number of throws for each target. It also excluded the possibility of position effects arising from progressive physical changes in the dice. Each target face occurs an equal number of times in every position, so that the effects of changing face frequencies would cancel out. In this Latin Square series Dr Thouless obtained a just significant result (odds 40 to 1), indicating an excess of willed faces.

In the second series the targets were determined by hand shuffling 6 cards numbered 1 to 6. The targets were taken in the order of the numbers down through the deck. A novel feature was that Dr Thouless performed the throws blind: he did not look at the cards to find out at what target he was aiming until after he had made the throws. He called it a psi γ experiment because apparently ESP and P K were both involved: ESP to find out the target and P K to produce the excess of target faces. In this series Dr Thouless obtained only an insignificant excess of target faces (odds 14 to 1), but there was also a definite position effect, the first run of each session giving a substantially higher score than the other two. These results were not very striking from the statistical point of view, and it was unfortunate that Dr Thouless, who was himself acting as subject, did not have a second person to make an independent record of the dice throws so as to exclude the possibility of systematic recording errors. The real value of the tests lay in the excellence of their design, which might well serve as a model for future workers.

G. W. Fisk's Experiments

The next important development was Mr Fisk's application of the concealed target technique to distance tests of P K (14). The ten subjects who took part threw dice in their own homes, aiming

at target faces chosen by Mr Fisk in Surrey. The subjects did not know what the targets were, so any recording errors could not (without ESP) be related to the targets and could not cause extra-chance scores. Mr Fisk decided the targets by throwing a die. The targets were usually changed each day, but sometimes they were not, with the consequence that the subjects did not throw the same number of times for each face. The distances varied from three to three hundred miles, the most successful subject, Dr J. Blundun, being 170 miles away in Devon.

The results were highly significant, the odds given for the total score of all subjects being 4,000 to 1; but in working out this figure no correction was made for the unequal target distribution. There was a preponderance of sixes among the targets, and if the dice were biased in favour of sixes, this would account for some of the deviation. Actually, it is clear from the raw data given in the report that the correction would be relatively slight and insufficient to affect the striking significance of the results, but it is unreasonable to expect the reader to make the calculation. The report stated that experiments were still in progress, so doubtless when the final version appears the necessary correction will be made. An interesting feature of the results was the outstanding score of the most successful subject, Dr Blundun, who did a large number of throws which produced a score corresponding to odds of 30,000 to 1. In her case she went on long enough for the target frequencies to even out, so that hardly any correction is called for. She might almost be said to be a PK star, similar to the specially gifted subjects found in ESP tests. Since the publication of the report Dr Blundun has done some more dice throws, with chance results only, but unfortunately she became ill and is too incapacitated to continue tests for the time being. There was another interesting point about these PK results. Mr Mitchell conducted a series of PK tests independently of Mr Fisk. In these the subjects threw alternately for 'high' and 'low' faces, but with only chance results. It may be that Mr Fisk is a particularly successful experimenter in both ESP and PK tests.

The Fisk-Mitchell report was largely taken up with the application of a divergence scoring system to PK data. In one way this was a pity, because it rather drew attention away from the main point of the results, namely the unusually clear and substantial positive scores produced in Mr Fisk's distance PK tests. On the divergence scoring system a throw that resulted in the target face coming to rest uppermost was given full points. If the target face came to rest at the side of the cube the throw was given half points, and no points at all were given if the target face was underneath.

On this system of scoring some of the data yielded higher odds, suggesting that there was, in addition to the fully successful throws, an excess of throws in which the dice came to rest partially orientated.

Experiments on Plants and Paramecia

It may be wrong to think of PK as an actual force pushing the dice into a selected position, for there is no evidence that 'mental force', as it has sometimes been called, will depress a balance or deflect a needle. The PK effect has only been observed in connection with dynamic situations of indeterminate outcome, such as dice throwing and coin spinning. There are, however, some experiments suggesting that PK may influence living things. Mme Vasse of Amiens claims she has been able to influence the germination of seeds by mental concentration (26). Mr Nigel Richmond, working in London, claims to have influenced by PK the movements of microscopic pond creatures called paramecia (19). The introduction of living matter naturally complicates the issue by increasing the number of uncontrolled factors, and Mr Richmond's method is unlikely to comment itself to an experimenter wanting a relatively swift and certain manifestation of the PK effect.

Mr Richmond placed a drop of pond water on a glass slide and viewed it under a microscope. The eyepiece of the microscope was fitted with cross wires that divided the field of view into four quadrants. He moved about the slide until a paramecium that was making more or less random movements was sighted at the point of intersection of the cross wires. He then selected one of the four quadrants by turning over playing cards, and 'willed' the paramecium to swim into the selected area. If after fifteen seconds (timed by stop-watch) the creature had swum out of the field of view through the selected quadrant, or if it was still inside the quadrant, this was a 'success'. Paramecia that landed on the cross wires were discounted, and paramecia that went into any of the other quadrants were 'failures'. On the average there should be one in four successes if only chance is operating, but Mr Richmond obtained a highly significant deviation from chance expectation. The weak point about these experiments, which in my view makes the results unacceptable, was the absence of adequate provision against errors of observation and recording. If systematic errors can sometimes vitiate the recording of dice throws, they are even more likely to affect the relatively complicated observations of the movement of paramecia. In these tests Mr Richmond acted as his own subject and did most of the recording. He had a fairly

complex task to perform—sighting the paramecium, lining it up, choosing a target, setting the stop-watch, following the animal's movement, following the time, noting where the paramecium disappeared from view or where it was after fifteen seconds, and finally recording both the actual quadrant position and the target quadrant. During this period, although he must constantly watch through the microscope, he must avoid knocking or shaking the instrument in the slightest, for this would alter the field of view and also the apparent position of the paramecium. It is of interest that when a lower magnification was used, which would widen the field of view and slow down the apparent movement of the paramecia, presumably making them easier to follow, the scoring fell off. Dr F. W. Knowles repeated the experiment with paramecia, but failed to reproduce Mr Richmond's results. He also tried many dice tests, and attempted to influence by PK the germination of seeds, the clotting time of blood, and the sedimentation rate of red blood cells, but all without success (11).

IV. PSYCHIC HEALING

Scientists in other fields apply the experimental approach to the most diverse problems, but for some reason psychical researchers tend to limit their experiments to standard tests of ESP and PK. Dr F. W. Knowles broke the shackles of tradition and used experimental methods in the investigation of psychic healing. The work began many years ago when he tested his own powers of relieving patients of organic pain by means of mental concentration. His efforts met with some success, notably in cases of osteo-arthritis (18). Next he tried to relieve the pain produced by applying a tourniquet to the arms of volunteers, but in this he failed. Later he applied mental concentration, and also hypnotic suggestion, to standardised, artificial burns, but again with no consistent effect. These poor results with artificial conditions contrasted with the substantial subjective relief experienced by patients who were actually ill. Some advanced cancer patients got up and walked about after treatment, although they all died eventually. It occurred to Dr Knowles that perhaps sick patients were particularly responsive to suggestion. To test this he carried out a number of mock treatments in which he behaved outwardly as usual, but did not apply the mental concentration process. On these occasions patients who had hitherto obtained regular relief, and had every reason to expect relief again, gained no benefit. A challenging observation! One hopes the study will be continued. As Dr Knowles points out, this branch of parapsychology has immediate practical utility (11).

V. CRITICISMS BY MR G. SPENCER BROWN

This survey would be incomplete without some discussion of the criticisms recently published by Mr G. Spencer Brown, lately Perrott Student in Psychical Research at Trinity College, Cambridge (2). Most of his criticism strikes me as completely fallacious, but it sounds impressive, and may puzzle those unfamiliar with practical experiments. Mr Brown shifts his ground when challenged, and his arguments are rather confused, so that it is difficult to summarise what he has to say. He appears to make at different times three separate criticisms. (1) Randomisation of targets in ESP and PK experiments is imperfect because random number tables are defective. Statistically significant results of the same order as those obtained in parapsychological research have been obtained by Mr Brown by matching columns of numbers taken from random number tables. (2) 'Control' tests often give as significant results as the actual experiments, which shows that the effects are due to the inherent non-random behaviour of cards and dice, and not to ESP or PK. (3) The logical basis of the accepted concepts of probability is faulty, hence the peculiar empirical effects. The last point is really a philosophical question, namely whether the axioms on which mathematical theory is based are universally valid. I am not competent to make any comment on this, and it is not a criticism of particular relevance to psychical research. Modern science as a whole is dependent on statistical assumptions which in practice have been found to work well in many different fields. That control tests give results comparable to those of actual ESP experiments is an absurd claim that could only be made by someone ignorant of or wilfully blind to the facts. Cross checks in which calls are matched against target sequences from other runs have not yielded consistent deviations, nor have matchings of packs of shuffled cards, although both have been given extensive trials. Mr Brown quotes the example of Coover, who found extra-chance effects in telepathy tests even when the agent was not looking at the cards. This was not a control test in any reasonable sense, for it has been shown that some ESP subjects do not need an agent. A proper control test is one in which some irrelevant sequence is substituted for either the call sequence or the target sequence, and such tests do not give comparable results.

In PK tests the situation is slightly different. Unlike the arranged targets in card tests, dice throws are not necessarily random, since no die can remain permanently unbiassed. The case for PK rests on the observation that the behaviour of the dice

changes according to the changing targets, and that this correlation only occurs when a PK subject is 'willing' it to happen. More control tests are needed in PK research to demonstrate the absence of a correlation when there is no subject 'concentrating'. Mr Brown quotes the example of the paramecia experiments to show that control data in PK tests give the same deviant scores as the 'willed' trials. In these experiments Mr Richmond carried out one series in which he did not turn over the card to select the target until after he had made a note of the position of the paramecium. Under these conditions he still produced deviant scores. It could be, as Mr Richmond supposed, that this was due to a combination of ESP and PK, similar to the effects in Mr Fisk's distance trials. On the other hand it could be due to faults in the experimental methods, such as errors in observing and recording, or defective randomisation of the targets. Repeated experiments under improved conditions, and proper control matchings against completely irrelevant targets, would settle the matter.

Mr Spencer Brown's first point, the inadequacy of random number tables, is a matter of simple factual observation open to anyone to check. In view of the tests for randomness applied to such tables before they are issued, it is hard to believe that consistent deviations can really be produced by matching successive columns of numbers. Until Mr Spencer Brown sees fit to publish the details of his alleged findings, this part of his criticism is best ignored.

In a talk given in the B.B.C. Third Programme on 21 February 1954, Mr Brown stressed another criticism, namely the ephemeral, unpredictable, and unrepeatable character of psi effects, which suggests statistical freaks rather than lawful phenomena. The assertion includes a dangerous half-truth. Though different from the purely chance data of control tests, the effects given by unselected subjects in mass experiments are admittedly slight, variable, and uncertain. But the results of specially gifted subjects like Shackleton and Stewart are obviously consistent and lawful even on the most casual inspection. For example, the ESP scoring of both these subjects stopped dramatically whenever the agent ceased looking at the cards, and each of them scored consistently better with certain agents. Shackleton was able to focus his scoring on the contemporary card or the card one ahead according to the experimenter's directions. Such meaningful responses to changes in conditions prove that the effects are not freaks of chance, but Mr Brown makes no mention of this type of evidence. His broadcast will have had the effect of misleading the general public.

VI. CONCLUSION

This brings to an end the present survey, from which it will be seen that British parapsychological research has not done too badly. Far from stagnating from lack of results, as seemed not unlikely six years ago, the work has gone ahead more rapidly than before. But progress is precarious. Most of the British research continues to be done by independent individuals working outside the universities. The number of persons who have the necessary scientific background and also the opportunity to spend a long time on these investigations is naturally very small. One day there may not be any at all. The yield in the last few years would have been poor indeed but for the work of one individual in particular, Mr G. W. Fisk. About a dozen persons have been responsible for most of the research described in this survey. The future hangs in the balance. Unless the subject attracts skilled scientific workers, and unless money is forthcoming to support these workers, swift progress cannot be expected.

Apart from these considerations, the uncertainty and unpredictability of psychic effects is an obstacle that discourages many potential experimenters. The difficulty is apt to be exaggerated. At one time some British experimenters despaired of ever obtaining results as successful as those reported from Duke University, but in recent years British results in both ESP and PK experiments have been, if anything, more spectacular than the Duke claims. Given time, patience, and sound technique, parapsychological experiments are by no means so unfruitful as certain critics would have one believe.

One consequence of the dearth of skilled experimentalists is a tendency to keep to well-tryed routine tests, instead of inventing new methods to deal with wider problems. The work done in the recent past consists mainly of card calling and dice throwing. While these methods have by no means exhausted their usefulness as instruments of discovery, there is room for other types of investigation. Experimenters need to find ways of tackling anecdotal observations and spiritualistic phenomena more appropriate than card guessing and more scientific than simple reporting. The present survey included one experimental study of psychic healing, one psycho-analytically orientated ESP experiment, a few studies of object-reading, and one EEG test with a trance medium: but what a paltry collection in view of the enormous scope for original research!

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REVIEWS

PSYCHICAL RESEARCH TODAY. By D. J. West. London, Duckworth, 1954. 140 pp. Illus. 12s. 6d.

The large number of members who have recently joined the S.P.R. are fortunate in having to help them in their studies two surveys of the subject that are compendious and up to date: Mr Antony Flew's *A New Approach to Psychical Research*, reviewed in the March issue of this *Journal*, and this book of Dr West's. It is to be hoped that both authors, who may look forward to many years of activity in psychical research, will later give us books covering the same ground in greater detail.

Dr West is both a medical psychologist and an experienced investigator of ESP, and these qualifications make the Chapter (III) in which he discusses the psychology of mediumship, and Chapters V, VI, and VII, devoted to the origins and development of ESP research, particularly valuable.

In his Introduction he reminds us of the universality of the belief in psychic phenomena of various kinds, and pays tribute to the founders of the Society who in the face of ridicule and hostility pioneered the systematic investigation of them. But he does our founders less than justice in attributing to them (p. 16) an optimistic confidence that 'when the right number of "cases" have been published, they will carry conviction to a sceptical world'. He contrasts this with the concentration on experimental work in the last few decades. But, as the early volumes of *Proceedings* show, experimental work in various directions *was* carried out by the founders and their fellow members, some of it admittedly faulty in method or inconclusive in result, but fruitful enough, to take a single example, to lay 'the foundations on which the psychology of abnormal mental states during the next twenty years was to be based' (T. W. Mitchell, *Proc.*, xlv, 179).

If, however, invidious comparisons between past and present are avoided, the quantitative work of the last twenty years deserves the praise Dr West gives it. It could not have been achieved without great ingenuity in devising fresh methods and great pertinacity in pursuing them. It is reassuring to hear from one of Dr West's experience that 'the current methods in ESP research are superior to most of the experimental methods used in psychological laboratories' (p. 86). With the supposed statistical flaws, which have received so much attention lately, Dr West deals very firmly (pp. 82, 83) pointing out the trouble psychical researchers have taken to test the validity of the methods they use. I should not myself go so far as Dr West does when he claims (p. 7) that

recent experiments have changed 'the whole character and status' of psychical research, but they have certainly changed the status of some of the faculties studied. Twenty years ago the evidence for precognition and clairvoyance was almost negligible. With the Soal-Goldney report and Pratt's experiments with Pearce in mind, no one could nowadays reasonably say that, though both faculties seem to raise such appalling theoretical difficulties as to require proof piled on proof before they can be accepted without reserve.

Repeatable experiment, that invaluable instrument of many of the natural sciences, is not yet at the service of psychical research. Until it is, the validity of research in our subject must depend on the competence and integrity of the investigators. With the passing of the years great reputations fade, and the precognitive eye can envisage Dr West, as he delivers his Presidential Address, shaking his grey hairs over the obstinate incredulity of a younger generation that fails to attach a higher value to the quantitative experiments of the present decade than he himself now does to the qualitative work done during the first half of the Society's existence.

As its title suggests, the book is mainly concerned with the experimental work which is the principal concern of contemporary enquirers, but Dr West finds space to touch on several other branches of our subject. His chapter (II) on seance room phenomena seems to me well balanced, and in the following chapter on the psychology of mediumship he reminds us of the very different view of dissociated states taken by modern psychologists and psychical researchers from that prevalent when Myers wrote his great book. In another chapter (VIII) he has short notes of interest on dowsing, haunts, poltergeists, stage telepathy, and other matters. He concludes with a chapter, which might well have been longer, on various theories of telepathy and precognition. Throughout the book Spiritualists receive some hard knocks, mostly well deserved. Belief, however, in survival has not been confined to the credulous or to persons unacquainted with psychical research, and if this difficult problem was to be discussed at all, it should have been given less summary treatment.

W. H. S.

NEW DIMENSIONS OF DEEP ANALYSIS: a study of telepathy in interpersonal relationships. By Jan Ehrenwald, M.D. London, Allen & Unwin, 1954. 316 pp. 25s.

This latest psycho-analytical approach to psychical research presents in a somewhat complicated analytical terminology many ideas which will be familiar to readers of this *Journal*. Dr Ehrenwald considers that psi belongs to the level of the id. In common

with other unconscious mental functions, psi fails to conform to the logic of the ego. 'On the id level the laws of contradiction are no longer valid. A statement may be both true and false. Love and hatred may stand for each other . . . Black may be white. . . Temporal order and sequence have lost their meaning ; so have spatial relationships. The notions of past present and future are hopelessly jumbled' (p. 126). Hence the vague and confused nature of many psi impressions, which makes it difficult to isolate clear-cut cases.

Dr Ehrenwald believes that even a superficially confused impression can be identified as psychic by means of 'tracer effects'. He discusses these tracer effects or identifying criteria at great length. First, the criterion of uniqueness. The element or *motif* of the telepathic impression 'must be of such a unique kind that its simultaneous appearance in the two presumably corresponding sets of mental events cannot reasonably be ascribed to chance'. In other words the coincidence must be impressive. Alternatively the telepathic impression may contain 'such a multiplicity of distinctive features as to leave little doubt as to its specificity in the two corresponding sets of mental events' (p. 29). The last criterion is slightly out of line with psychical research tradition. The impression may have psychological significance. It is a characteristic of spontaneous psi that it emerges in response to a deep emotional need. Dr Ehrenwald gives the example of one of his patients who dreamed of an apartment strikingly similar to his own, although 'she could not have learned of these details by any conceivable sensory means'. In this case the psychological motive activating the psi was found to be 'the patient's unconscious desire to move into the therapist's home and to be adopted as a daughter by her father substitute' (p. 31).

Psi having been convincingly demonstrated in experimental tests, one might reasonably expect to find examples of it in everyday life. In fact it was the occurrence of suggestive spontaneous experiences that first gave rise to the concept of psi. Unfortunately, spontaneous experiences are usually too complex to lend themselves to clear interpretation. Factors such as chance coincidence, subliminal perception, and inaccurate reporting cloud the issue in all but a few exceptional cases, so that hardly ever can one pin-point a particular instance of spontaneous psi with any degree of scientific certainty. Dr Ehrenwald would not agree with this. Armed with the criteria previously mentioned, he has little hesitation in identifying the operation of psi in many different human situations.

In child-parent relationships, for example, Dr Ehrenwald finds

a clear field for the operation of psi. All psycho-analysts recognize the great sensitivity of young children to their parents' attitudes. By a sort of emotional contagion the infant picks up far more of the parents' mental traits than can be easily accounted for by verbal instruction. Particularly is this the case in the field of unconscious mental life. Dislikes, phobias, guilt feelings, and neurotic symptoms generally pass with extraordinary ease from parent to child. The same thing sometimes happens within a family group in which several persons have inter-related neurotic symptoms. The symptoms of one member of the group dovetail with those of another so that, for example, a son's failure to assert his independence satisfies his father's emotional need to dominate. Dr Ehrenwald sees in such complimentary neuroses evidence of the operation of psi in both the reception and transmission of unconscious impulses.

Castration fears, Oedipal jealousies, and all the other fantasies that feature so prominently in psycho-analytic material are believed to be acquired in earliest infancy. At the same period the child incorporates into his own super ego standards of thought and behaviour derived from the parents. How does all this happen? Do instruction and imitation adequately account for the completeness of the process at such an early age? Dr Ehrenwald suggests that psi, operating on both ego and id levels, is an important medium whereby the individual receives the innumerable clues upon which depends his conditioning to the intricate fabric of our culture.

In an amusing diversion from his main theme, Dr Ehrenwald considers the operation of telepathic leakage in the genesis of spurious psychological evidence. He mentions the old phrenologists, and also the mesmerists, whose fantastic theories were apparently confirmed by the behaviour of their somnambules. He also gives instances of patients' dreams conforming suspiciously closely to the theoretical preconceptions of their particular therapists. Such observations have hitherto been ascribed rather loosely to the operation of suggestion from practitioner to patient, but there might well be a telepathic factor.

While not necessarily subscribing to Dr Ehrenwald's faith in his criteria for identifying psi, one may give complete support to the attempt to consider theoretically the possible role of psi in nature. Speculation on these lines may yield valuable ideas for further research. For example, could bird homing be due to the operation of psi? Methods of testing the hypothesis at once spring to mind, and the current researches of Dr J. G. Pratt may decide the matter. But the suggestion of a psi basis for homing is no more than the first step towards appropriate investigation. The danger of

speculative psi theories lies in mistaking plausible interpretations for positive evidence. Now Dr Ehrenwald speculates long and well, and finds a great many psychological observations that could be attributed to psi. But although he rarely gets beyond the formulation of interesting possibilities, he presents his interpretations very ingeniously and confidently so that the reader must constantly keep in mind the need for further confirmation if he is not to be carried away by the author's enthusiasm.

D. J. WEST

THE JOURNAL OF PARAPSYCHOLOGY. Vol. 18, No. 1, March 1954.
Durham, N.C., Duke University Press. \$1.50.

Dr Rhine's editorial deals with the situation that has arisen as a result of the virtual disappearance of external attacks on parapsychology and with the necessity that parapsychologists should continue to resist any temptation to lower standards or to substitute theoretical speculation for experimentation.

Dr West makes a survey of recent work in experimental parapsychology in Great Britain.

Dr Forwald reports results of an experiment on PK placement in which it was his object to find out whether there was a consistent decline effect within his experimental series. In the first part of his experiment, when his subjects were unaware of this object, he found a highly significant decline effect ($P=.00006$) which, however, disappeared after it had been pointed out to his subjects.

Dr Pratt gives brief and easily understood instructions for using the Greville method of assessing results for multiple calling of a single pack in ESP experiments. This is to be welcomed since those using this method have hitherto found it difficult to discover what is the correct method of treating their results.

There is a review by myself of Rhine's book, *New World of the Mind*, and a very thorough and interesting review of Dr Johnson's *The Imprisoned Splendour* by Professor Price.

R. H. THOULESS

CORRESPONDENCE

'A NEW APPROACH TO PSYCHICAL RESEARCH'

SIR,—Mr Mundle, in his thorough review of my *A New Approach to Psychical Research* (this *Journal*, No. 679), puts his finger on several serious faults; most important perhaps the failures

to bring out 'that it is a matter of choice or convention what we take as our "level of significance" ' (p. 283), that 'to reach significance, at the chosen level, the rate of scoring and the number of trials are interdependent' (p. 283), and the 'significance of the declines reported in the American work' (p. 284). I have no wish to try here to extenuate my faults nor even to defend positions attacked which seem to me defensible, merely because I think them still defensible. But there are one or two points where by clarification I can perhaps help discussion forward.

(1) *Survival and decision issues.* I wrote :

It is because . . . it is possible to be *mistaken* as to whether one did or suffered something, whereas it makes no sense to talk of being *mistaken* as to whether one is now in pain, that one cannot get around these points about *decision* issues by, as it were, appealing to a possible incorporeal being . . . to settle expertly whether or not 'he' (or it) is Myers (p. 82).

Mundle comments :

Surely it is not necessary to insist that the memories of a (hypothetical) surviving spirit must be *infallible* before it could qualify to be called 'the same person' as a certain deceased mortal! (p. 282).

No. The point, which must have been badly expressed, is that questions about personal identity are not like questions about pain, in which the honest testimony of the subject necessarily constitutes the final word ; and that, as the early critics of Locke insisted, memory may *reveal* but it cannot *constitute* personal identity (even though, in some way excessively difficult to specify, the capacity to display appropriate apparent memories may be some *part* of what is meant by 'same person' and suchlike expressions).

This complex of problems is not made any easier by certain features of our everyday vocabulary. First, true memory involves personal identity ; in that 'I remember X' entails 'I (am the same person as one who) once learnt or experienced X'. Second, though memory notoriously plays tricks, in the dominant sense of 'remember' remembering is as far as it goes infallible ; in that 'I remember X' entails 'X occurred', and if you misremembered then it's not remembering but 'remembering' (in snigger quotes : cf. the difference between knowing, and 'knowing' wrong). Third, perhaps because memory is extremely reliable on the whole and as far as it goes, we do not systematically distinguish between *apparent memory*, which may or may not be veridical, and *true memory*, which is by definition veridical.

Thus it seems more reasonable than it is to say :

If, after Flew's body) has grown cold there occur experiences involving memories of many incidents in the life of (the embodied) Flew, surely the subject of such experiences would have good reason to decide that he (it) was in *one* sense the same person . . . (p. 282).

If these were true memories then necessarily he *is* the same person (First and Second points): but the question precisely is whether in such circumstances apparent memory can be true memory (Third). Though perhaps this possession of appropriate apparent memories is *part* (though not surely a necessary part) of the meaning of personal identity. And certainly 'no one else (except an omniscient deity) could be in as good a position' to know what apparent memories 'he' (it) had: to settle what Mundle (I think *most* misleadingly) calls a 'decision issue . . . concerning non-linguistic facts' (p. 282).

(2) *Guesswork*. My purposes in recommending that, at least as a stopgap, we think of psi-gamma as a species of guesswork were: first to avoid the difficulties generated by other even less satisfactory models; and, second, 'to suggest a few possibly useful questions'. Mundle objects: 'it would be necessary to distinguish forthwith between normal and paranormal guessing' (p. 285). But might not part of the value of the model be precisely this, that it forces us to remember that we have no sure way of making this distinction save by looking beyond the guesses to their correlation with the targets? (Cf. Spencer Brown). Though Dr D. J. West in an earlier correspondence said, 'In a successful ESP test, there is a correlation between the cards and calls which shows that one of the associations available to the subject, and effective in determining his choices, is the target order' (this *Journal* No. 671), all that has actually been *shown* is that the correlations occur (though Dr Schmeidler has given some clues for spotting high scores in advance and some of Dr Soal's work with his star subjects suggests a stronger verdict). And again, though investigation may be at present impracticable, one would like to know more of the mechanisms of guesswork in which no psi-factor seems to be involved.

However, I do not wish to defend *à l'outrance* what was never more than a tentative suggestion: its first purpose could usually be as well or better served by the non-committal psi-terminology; and the second is very likely misguided.

(3) *Psi-concepts as essentially statistical*. I argued 'it would make no sense to speak of psi-gamma in connection with any *single* item of correspondence between a putatively paranormal dream . . . or what not, and what had happened . . .' (p. 118).

Mundle objects that this is inconsistent with my accepting that

a psi-factor is operating in certain spontaneous and mediumistic cases. But surely this is not so. For in any impressive cases there will be correspondences between a whole *series* of items: the dream will have occurred at or near the time Aunt's ship went down, the ship in the dream will resemble the real ship in several respects and so on. Incidentally this suggestion about psi-concepts is one which does 'seem to be the fruit of linguistic analysis' (p. 288).

ANTONY FLEW

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THE DESIGN OF EXPERIMENTS IN PSYCHOKINESIS

SIR,—Recent correspondence in the *Journal* has reintroduced the subject of psychokinesis, mainly in relation to Mr G. Spencer Brown's views on the application of probability theory to the experimental data of psychical research. With this aspect I am not concerned in this letter, but there are one or two remarks about PK that may be worthy of comment.

In the January-February issue Dr Soal criticizes Mr H. Forwald's experiments on the ground that 'the proximity of the experimenter may have unconsciously biased the apparatus'. The criticism may be just, since no apparatus can be perfectly steady or constant in its properties; but it ought to be said in Mr Forwald's favour that—no doubt to reduce the risk Dr Soal has in mind—his table was placed on a concrete floor. This does not altogether dispose Dr Soal's doubts; but it does imply that the experimenter was not lacking in care.

On the other hand, I would like to suggest there is a more serious ground for anxiety. Except for a few short occasions Mr Forwald filled the double role of subject and experimenter in his own researches. All the work involved in controlling the electrical apparatus, in placing the dice in the container, in counting the score and making the record was done by the subject himself, unwitnessed.

At what stage in the difficult history of psychical research it became permissible for sensitives to report their own results and expect them to be accepted as serious evidence in psychical research, I do not know. The number of such reports has grown disturbingly in recent years. In fairness to Mr Forwald, however, it may be said that when he came into the quantitative research field some five years ago the practice of sensitives acting as sole observers of their own phenomena had become thoroughly

established by experimenters who might have been expected to show a deeper regard for the standards of evidence established by workers in the Society many years ago.

On another point in PK, Dr Soal, I suggest, gives a rather too rosy picture of PK experimental designs when he states :

Mr Brown still seems unaware of the exact procedure adopted in a standard dice-throwing experiment since in his second letter to *Nature* (26 September) he asks if the target numbers are to be chosen by more dice throws. Actually, the subject tries in blocks of 24 trials for each of the six faces in turn, but the experimenter decides the order of the six faces by taking a row from a 6-6 Latin square. This simple method is adequate. . .

The statement reads as if the randomised design known as the Latin square were in common use in PK research. In fact, though the method is recommended in Dr D. J. West's *Tests for Extrasensory Perception : an Introductory Guide* (published by the S.P.R. in February 1953), in the published reports of PK experiments performed in the last twenty years, that design has been used only twice. The first occasion was by Dr R. H. Thouless who used it in one part of one of his experiments. The second was by Mr A. M. J. Mitchell and Mr G. W. Fisk who used it for part of the throws of one of their ten subjects.

In the March-April issue Mr C. W. K. Mundle proposes a test of one of Mr Spencer Brown's beliefs about dice-throwing. If I understand Mr Mundle correctly he wishes to re-examine 'the Duke records' and from them to conduct a cross-check. In the records that are to be taken from the files, the data are to have the following properties :

- (1) The same set of dice are to have been used in two experiments.
- (2) The six targets shall have been equalised in each experiment.
- (3) The target order shall have been different in the two sets of data. For example, 1, 2, 3, 4, 5, 6 and 2, 1, 5, 3, 6, 4 would be different orders.
- (4) The data shall show a deviation from chance expectation—presumably a significant one.
- (5) There shall be 'position effects'—also significant.

No proposal for a cross-check of PK data could seem more reasonable. It may therefore cause surprise to Mr Mundle (who may not have read the literature in detail) to learn that no such data exist at Duke. This fact will be evident from a reading of the published reports.

Mr Mundle suggests that Mr Spencer Brown might prefer a different type of cross-check. Even so, if the conditions resemble (1), (2), and (3) Mr Spencer Brown will be thwarted for lack of proper data. Even (2) alone would be difficult to fulfil, for the kind of material Mr Mundle has in mind, in which the six targets were equalised, forms a minute proportion of the whole.

That Mr Spencer Brown has laid himself open to heavy criticism may be true; but it seems a pity to expect him to put his thesis to the test by asking him to make a cross-check that is *a priori* impossible to perform. There may be data elsewhere in the world that would meet some of Mr Mundle's conditions, but I should not like to assert this possibility with conviction.

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PROBABILITY EVALUATION AND ESP RESEARCH

SIR,—Mr Spencer Brown wrote in his article in *Nature* (No. 4369, 25 July 1953, p. 155): 'The concept of physical randomness upon which applied probability theory depends is necessarily vague in much the same way as the concept of physical simultaneity is vague.' Does this mean that the foundations of an axiomatic treatment of probability cannot be laid and the avoidance of metaphysics cannot be achieved by one of the methods recommended in modern mathematical statistics, e.g. the method of relating probabilities to a *Merkmalraum* or 'sample space' in which every conceivable outcome of an experiment is, by definition, completely described by one, and only one, 'sample point' and an 'event' is defined as an aggregate of 'sample points' (W. Feller, *An Introduction to Probability Theory and its Applications*, New York, John Wiley, 1950, Vol. I)? The insinuation that the basic probability concepts with which we work will not bear scrutiny is not new. E. A. Becknell, in the *American Journal of Psychology* (1940, 53, 604-9), spoke of 'probability as a function of ideology'. He argued that probabilities are not independent of one's state of knowledge or the thinking process and that, in the preoccupation with the calculus of probabilities, we tend to overlook the logic of probability. Mrs Martha Kneale (*Enquiry*, Vol. I, No. 1, April 1948, p. 25) complained that G. N. M. Tyrrell's expositions seemed to waver between a *Spielraum* (or Indifference) theory and a Frequency theory. Surely these are defects of exposition which are easily remedied by psychical researchers when they know where to look for the relevant mathematical information.

I suggest that Mr Spencer Brown's quarrel is not with probability *theory* but with its *application* to psychical research. He says (*op. cit.*, p. 156) that it should be a 'matter of some surprise that present probability theory applied to science in fact works as well as it does . . .' But if it works 'as well as it does' in several fields of scientific enquiry, what grounds can be alleged for saying that it does not work so well in ESP research? Mr Spencer Brown's criticism of 'control experiments' is more than met by Dr Soal's clarification of the term 'cross-checks'. When Professor Rhine first reported his 'card-calling' experiments, his use of the Binomial distribution $(p+q)^n$ for probability evaluation was challenged on the alleged ground that the subject's 'guesses' could not be treated as strictly independent of one another. D. L. Herr even suggested (*J. Experimental Psychology*, xxii, 1938, 491-6) that some method of evaluation like Tchebycheff's inequality, which is independent of the nature of the distribution, should be used. But quite apart from the fact that the gain in generality is secured only at the cost of obtaining inadequate information about the particular, statisticians showed that, while the variance for the Binomial case is different from that for Rhine's 'card-calling' situation, the difference is not appreciable enough to affect the statistical validity of Rhine's work. Since the standard error method provides but a crude indication of the difference between two samples of a population being explicable on the Null Hypothesis, psychical researchers have resorted, and very rightly, to the versatile X^2 test. The X^2 distribution is, of course, continuous while the Binomial distribution is not. But where the sample sizes are relatively small and the Binomial distribution is very skew, safeguards (e.g. Yates's correction) are easily found. Technical difficulties arising from the 'multiply-determined hits' in Dr Soal's experiments have been dealt with by Robbins and Greville. It is time that Mr Spencer Brown told us plainly how we have gone astray in using methods of probability evaluation in ESP research. Like Mr C. W. K. Mundle I am curious to know how Mr Spencer Brown proposes to apply his hypothesis in detail to the Shackleton records.

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DUNNE'S 'SERIALISM' THEORY

SIR,—In his paper 'Serialism and the Unconscious' in the January-February 1954 number of the *Journal* Mr G. F. Dalton

tries to show that Dunne's theory with certain minor amendments 'can be used to describe, and to account for, a wide range of psychological phenomena'. I had thought that the theory had actually died from lack of support from scientific quarters, but there is evidently still some life in it, which is also shown by the fact that Mr Antony Flew, in his recent book *A New Approach to Psychical Research* devotes a ten-page Appendix to a criticism of it. I have given considerable thought to finding the weak spots of Dunne's argument for an infinite series of Times and may perhaps be allowed to present my findings to the readers of the *Journal*, as my points of attack are rather different from those chosen by Flew, and by Professor Broad in his paper in *Philosophy* (Vol. X, 1935).

Dunne's regressive Time series starts with his introduction of Time 2 in which our common Time, Time 1, moves. His argument is simple and plausible: Time has length, measured in years, days, etc., so when the Now passes over these lengths it must move with a certain speed or velocity. Since velocity is defined as length covered in unit of Time, so that we have $V = L/T$, we must introduce a Time 2 (T_2) to be able to express in a physical formula the velocity of the movement of the Now in our common Time 1 (T_1). This velocity will then be equal to T_1/T_2 , both Times being measured in the same units, i.e. in years, days, etc.

I shall now try to show that this first step, the introduction of Time 2, is not only unnecessary but without meaning, and that consequently Dunne's Time-theory is built upon the sand.

We are so accustomed to measure time lengths by means of our watches that we are apt to forget that these lengths are defined by the duration of certain events, viz., 24 hours being the time that elapses during one revolution of the earth upon its axis, so as to bring the sun over the same meridian, or rather the average time for 365 such revolutions. Consequently the speed of a rotating body, say an electric motor, may be given without reference to time length at all, since for instance 1400 revolutions per minute means that the motor rotates $1400 \times 60 \times 24$ times as fast as the earth,¹ or 1400 times as fast as the second-hand on a clock. If we choose the speed of the latter as unit and call it one Rot, the speed of the motor would be 1400 Rot. Similarly with straight motion. We could choose the average speed of a point P on the earth as unit, say a point near the North Pole moving one kilometer per hour, i.e. one kilometer for every 15° the earth turns (on an average). Let us call this unit for one Ford (to honour Henry); we

¹ For the sake of simplicity I ignore here, and in the following, the small difference between one solar and one sidereal day.

should then measure the speed of motorcars in Fords, determined by counting the kilometers the car runs while the minute-hand on a clock makes one revolution, as this will correspond to P having moved one kilometer. Rotary speeds and velocities can thus be measured without reference to time lengths, due to the fact that time lengths are defined by means of the revolution of the earth upon its axis. If we ask with what speed this rotation occurs, the answer 'one revolution per day' is therefore tautological, and the only proper answer would be a demonstration, a pointing for instance to the second hand of a clock and stating that it rotates 1,436 times as fast as the earth.¹

We are now prepared to tackle the question of the Now's movement in the Time dimension. Let us provisionally accept Dunne's idea of this movement having a certain speed. Since everything moves in the Time 1 dimension with the same velocity, this cannot be measured, since it cannot be compared with anything. The movement is a unique natural event, and the question as to its velocity can only be answered by a demonstration. We may point to the second hand of a clock and say: please notice how fast the Now moves, while the second hand makes one turn, that is, while the Now covers a time distance of one minute. We shall then experience how fast one minute passes, which comes to the same thing as experiencing the length of one minute. Evidently there is no distinguishing between experiencing the velocity of the Now and the length of the time elapsed. In Dunne's language we may say that the velocity of the Now is one minute per minute. He actually arrives himself at this remarkable result. In *The Serial Universe* he writes that the velocity of the Now T_1/T_2 equals One, and then asks: One what? although the answer is simply one minute per minute, or—what is the same thing—one second per second, since he measures T_1 and T_2 in seconds.² But it is obviously not necessary to introduce a Time 2 to arrive at this result. T_1 divided by T_1 will do just as well.

In conclusion, I shall submit a further argument against Dunne's Time-theory, viz. that his reasoning that leads to the introduction of Time 2 is based on a false analogy. From the fact that Time—Dunne's Time 1—enters as a second dimension in the definition of velocity in space, Dunne concludes that what he conceives as the velocity of the Now in the T_1 -dimension must also be a two-

¹ A sidereal day equals 86,164 seconds or 1,436 minutes.

² Dunne refuses to give this simple answer and by some mathematical legerdemain arrives at the astounding result that the velocity of the Now equals the velocity of light (*The Serial Universe*, London, Faber, 1934, pp. 133-46).

dimensional affair and must therefore involve a second Time T_2 . This conclusion presupposes that T_1 is a true space dimension so that the relation R between L , length in space, and T_1 is the same as the relation R' between T_1 , length in the Time 1 dimension, and T_2 . This is obviously not the case. But if it were, if T_1 were a true space dimension, we should not need T_2 for a physical expression of the Now-velocity, for then LRT_1 would be a symmetrical relation, and just as we express velocity in space as L/T_1 , velocity in Time could be expressed as T_1/L , where L could be the length covered by our unit point P during time T_1 , or as T_1/r , where r is the rotations of the earth during time T_1 .

As the Time dimension is not a true space dimension, the question is what meaning, in physical terms, we can assign to the idea of a Now-velocity in the Time dimension. Psychologically the meaning is clear enough; when we experience time as passing slowly we over-estimate the physical length of time, and we underestimate it when we experience time as passing quickly. But physically the Now-velocity has no meaning. If it had, it should be possible to imagine what the physical consequences would be, if the Now-movement speeded up or slowed down. But how could the Now pass from January 1, 1954, to January 1, 1955, twice as fast, for instance, as it actually does? Only by the earth rotating twice as fast, but then the length of time covered in half the time would be halved and the velocity would be the same. If all movements in the universe doubled their speed we should not know it; but if we did, we should say that the speed of the movements had doubled, and we could perhaps with some justification say that time now passed twice as fast as before, since for instance we should—presumably—grow old twice as fast, but physically Dunne's Now-velocity would not have changed, as the lengths of Time defined by means of the rotation of the earth would be half their previous lengths.

These considerations show that a physical meaning of the Now-velocity, if there is any, must be expressed in terms of changes in space, and the only possible definition would be length of Time covered during some unit of movement in space, say one revolution of the earth upon its axis, so that we get the Now-velocity $V = T/r$, the same result I arrived at when considering Time as a true space dimension. If V is measured in days per one revolution of the earth the formula $V = T/r$ expressed in words says that the velocity is one day per one revolution, which comes to the same thing as the definition of the length of one day. So it would seem that, as far as physics is concerned, it is not possible to distinguish between length of Time and velocity of Time, the same conclusion

I arrived at when trying to answer the question as to the Now-velocity by demonstration.

AAGE SLOMANN

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DO WE KNOW WHAT WE ARE LOOKING FOR?

SIR,—To call our activity psychical research means we are looking for something, or believe we are. Have we any clear notion what it is? Or is the notion of some of us *too* clear? Many of the world's great discoveries (e.g. penicillin) have been made against the wish or expectation of those who made them, by some experiment 'going wrong'. How many discoveries have slipped back into the unknown because the discoverer's mind was so intent on the result he wanted that he failed to notice a more important one, or deliberately suppressed it because contrary to his wishes or to accepted knowledge or vested interests, or because it seemed to have no immediate exploitation value? The alchemists' pursuit of the philosopher's stone was not so ridiculous as till recently seemed: if uranium breaks down into lead, there seems no insuperable reason why we should not discover a reverse process. But, obsessed with too narrow an aim, they cast aside quantities of data which could have enriched them more than transmuting base metal into precious, and which certainly enriched science.

There are similar dangers in psychical research. The various parties are often more intent to prove the rightness of their own theories than to take in and examine facts. They incline to consider their own favourite experiments the only legitimate ones, and all others pointless or deluded. But the supreme stumbling-block to progress is muddled thinking as to aims. Generally speaking, those who believe in 'spirit' would find it hard to give meaning to the word. On the other hand, the position of those who reject 'spirit' is generally neither sounder nor less illogical than the spiritist's, since they usually eliminate in advance certain possible solutions to their search.

Let us attempt to clarify this statement. (If, as seems likely, we are exploring a world of thought, clear thinking is a technical *sine qua non*.) The disbeliever in 'spirit' presumably seeks a physical explanation for all phenomena. Yet he is confronted by 'telepathy' which shows no delay or weakening in time or space as we expect and measure when physical vibrations are involved; it may in fact foretell events that have not yet occurred in physical dimensions at all. In one breath he says psychic phenomena must be physical or nothing, in the next that they defy all physical laws. In affirming

that the law of inverse squares rules out any radiative explanation, he may be, *mutatis mutandis*, in a similar position to some eighteenth-century phlogiston partisan, or to some early physicist who asserted that all substances contract as temperature falls, and ignored the behaviour of one of the commonest substances, water. Do not psychic phenomena make it worth considering whether the 'law' of inverse squares is defective or exceptionable? It is now obvious that scientists of former ages, in order to maintain their position, treated facts that told against it rather as present-day science treats psychic phenomena. We must take care not to entrench ourselves in positions which are analogous but in psychical research harder to detect.

The few physicists who interest themselves in this subject must indeed have difficulty in finding any firm footing. Yet the advances in nuclear and quantum physics must have prepared them for the overthrow of many 'immutable laws' of older generations and qualified them to admonish materialist philosophers, psychologists, statesmen and others who still base arguments and theories on out-of-date science. If any physicist, forced to take account of psychic phenomena, still tries to fit them into known laws, or classes them as intractable outlaws, he may be persisting in blind alleys and preventing himself discovering important new laws or reforming old ones. Are not many of us trying to fit these facts into two, or even more worlds, or else into no world at all? Should we not bear in mind the possibility (fruitful in other fields than politics) that all phenomena belong to *one* world?

We may legitimately wonder whether some of our mathematicians and statisticians are not revolving in self-made grooves, only able to repeat already known tunes. They ask the world to accept that there is some power in man that 'gives rise to significant deviations from mean chance expectation in a series of guesses'. It is assumed that this something *is* at work in cards-cum-statistics experiments, and that it is the same faculty, or an analogous one, which enables a sensitive to make less circumscribed predictions, or statements characteristic of deceased persons. But what proof can be adduced for this assumption? No one knows which correct guess is due to this factor, so how is it proved that any are? Are we exercising it every time we guess, i.e. form an idea of something we do not know by normal means? Or only when we make 'correct' guesses? Is a first-class medium just a first-class guesser? No one seems to have any clear answers to such questions.

Nor, when we see the use and abuse of statistics in political and practical life, can we share the statistician's faith in his premises

—or results. For one thing, the precise moment (I use the word in more than its temporal senses) for starting or stopping a run of guesses must have great importance (the average of some small section of a long column of figures is seldom the same as that of the whole), yet it seems largely ignored. So also does the effect of the futility of the experimental means employed (we do not average a column of money by ignoring everything but the small change). We suppose the powers involved in guessing or prediction or clairvoyance to be of great importance and subtlety in the human make-up, whether incarnate or discarnate. Yet we assume they will employ themselves in footling, quickly-boring exercises while we largely shun experiments involving emotions and faculties (e.g. affection) which are fundamental in humanity.

'Chance', the supposititious standard in these experiments, is probably the most abused word in psychical research, and by the very people who most criticize credulity or slipshod vocabulary in others. They dethrone God or Great Spirit, and erect Omnipotent Chance—a logical and philosophical absurdity. Chance necessarily implies absence, nonentity, no-cause. Yet the Chance-ist makes his god the source and matrix of teeming macrocosms and microcosms whose complex relationships we have only begun to discern. He is usually a mechanist, yet unable to detect the hopelessness of his faith. For a machine is by definition a contrivance, and cannot exist without a contriver. Chance is no more satisfying, final or scientific explanation of cause than god or spirit. Rather less so: once God is found to exist, the rest is easy; but chance cannot even have existence, or somehow comes into being for each phenomenon it is supposed to produce, since it can be neither law nor subject to law. In any case, many things attributed by our fathers to chance have since been found conforming to law. We must be more sceptical vis-à-vis this Big Medicine. Does it gain authenticity when adorned with fresh, lengthier appellatives like Non-Causal Synchronicity?

The psychologist, who of all specialists should have the greatest interest and aptitude for psychical research, may be holding the most equivocal and obstructive positions of all—or if not, many philosophers and others who utilize his theories and jargon are even more oddly situated. For in his wake we glibly divide each others' minds into conscious, subconscious, superconscious, without stopping to enquire whether such divisions have ever been scientifically shown to exist, or whether we have demanded of their proponents such standards of proof as we demand for the hypothesis that man's 'spirit' is detachable from his physical body and may survive it. Many who calmly swallow the psychologist's

stories of the curious, not to say preposterous goings-on of our subconscious, would never dream of admitting that such a creature, already so independent and unknown to our sensory organism, might continue so after the latter's death. They stretch these gifts from the psychologist to fantastic limits in devising for psychic phenomena 'rational', 'consistent', explanations, supposed to be within the bounds of scientific possibility while survival is not, on no other basis than the psychologist's assurance that his theories have scientific basis : opinion upon opinion, precept upon precept.

Surely much material from good mental mediums makes nonsense of the hypothesis that it comes from some 'level' (existence and location unproved) of the sitter's and/or medium's mind? Who or what makes the *choice*, in all the matter available in a mind for reading (any number of unproved assumptions are involved in the hypothesis that one 'mind' *is* 'read' by another), of just the detail which the sitter's conscious mind recognizes as strikingly, even shakingly, characteristic of a dead person, or which it does not know until after the sitting? To exclude a 'spirit' source postulates some extremely ingenious (not to say mischievous or unscrupulous) department of the medium's mentality, perhaps conniving with a corresponding part of the sitter's, intent on deceiving the conscious mind of sitter and, possibly, of medium as well. Is such a situation *really* less fantastic than the hypothesis that such details are supplied by the memory or wider knowledge of a departed friend? From another aspect, if mediumistic material is merely the product of some shrewd, multifarious layer of the medium's mind, why are its predictions and advice so often ambiguous or defective as well as being so often strikingly right? Would not this entity, capable (according to some hypotheses) of prodigious exploits of research, detection and deduction, be expected to do much better with prophecy and advice than is usually the case? If some part of the medium's mind merely is at work, it is a poor money-maker : the shekels would roll in much more abundantly were it as reliable as it ought to be if possessing the powers sometimes attributed to it.

The very use of 'sub' and 'super', implying location, inferiority, superiority, may be preventing more exact realization of the nature of these faculties, just as the use of the word 'spirit' may rouse obscurantism in the materialist on the one hand or the spiritist on the other. Philosophers are not the only men who risk losing reality or fruitful ideas beneath masses of words. The curse of Babel lies not merely in confusion of tongues but, just as seriously, in confusion within a tongue, arising from conflicting uses of the

same word by specialists and propagandists. There is, too, more than a tendency to attach neat, Greek-derived tickets to phenomena and to think we have thus explained them, or on the other hand, to shun certain avenues of exploration because they habitually bear labels to which we are allergic: 'give a dog a bad name . . .' Both nominolatry and nominophobia can be present in the same patient. We have to cultivate awareness that our words are not last words, not explanations which end discussion and search, but labels to be discarded if found inadequate or untrue. We all have to try to divest ourselves of racial, professional and other prejudices and superstitions such as Tyrrell, perhaps with the clairvoyance of those about to die, pointed out at the end of his life. It is easy to discern and criticize motes in popular beliefs or in other specialists. But beams in our own eye are harder to cast out.

It is a delicate matter for the layman to point to occasions of error in the fields of specialists. Yet he, being free from their professional prejudices as to possibility and impossibility, and having perhaps a smattering of other branches of knowledge and their correlation, may be in a legitimate position for reminding them that in all its history science has never yet found any walls to discovery, though at every stage there were official scientists and official priests who declared they had reached the *nec plus ultra* of knowledge, human and divine. Perhaps fewer today say this explicitly, but just as many act and think as if it were true. It is essential to progress that searching eyes and fingers should constantly be moving forward. But their groping must not be confined within blinkers. They also have to be watchful sideways, upwards, downwards, backwards, inwards. Not all the lore of marginal seekers, or of past sages, or of insight, is irrelevant. Nor can any sector of the knowledge front advance without modifying the shape of the whole, or without support from the other sectors, or without drawing them after it.

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OBITUARY: COUNT PEROVSKY-PETROVO-SOLOVOVO

COUNT Perovsky-Petrovo-Solovovo, then Mr Michael Solovioy, joined the Society in 1890: after he succeeded to the title of Count he was, through ignorance of correct Russian usage, generally

known in the Society as 'Count Solovovo'. When he joined, the Society had just begun the collection of cases for the Census of Hallucinations, the report on which was published in Vol. x of *Proceedings*. His collection of Russian cases for the Census marked the beginning of an activity in psychical research which continued so long as his health permitted. His main interest, however, lay in 'physical phenomena', both as investigator and as critic of the problems of evidence specially raised by that branch of research.

For example, among the mediums he investigated was the Russian, Sambor (died 1902), whose phenomena greatly impressed him, as they suggested the passage of matter through matter. But he later discovered to his horror that one of the controllers, a personal friend of his own and a man of high social and official standing, who was supposed to be controlling one of the medium's hands, was not doing so. When pressed by Count Solovovo the friend most solemnly denied any intention to deceive, but Count Solovovo did not believe him. The friend's action threw suspicion not only on the genuineness of these particular phenomena, but on the trustworthiness of other records in which the *bona fides* of some one investigator or controller was crucial. In describing his experiments with Sambor (*Journal*, xxx, 89) he said, 'Who can tell how often such incidents may have taken place elsewhere without being known?'

He early took an interest in the controversy on Eusapia Paladino, and in 1910 had an opportunity of investigating her himself. In the preceding year the Feilding-Baggally-Carrington report, favourable to her genuineness, had been published in *Proceedings* xxiii, but reports in the contrary sense were also current. To set, it was hoped, the question at rest Count and Countess Solovovo co-operated with Feilding and W. Marriott in a short series of sittings at Naples. In the report (*Proc.*, xxv, 57-69) on these sittings he declared himself favourably impressed, though not absolutely convinced, by some of the phenomena, but very unfavourably impressed by the medium's determined resistance to effective control.

In the early days of psychical research the question had been raised whether, when observers too competent to be taken in by ordinary tricks reported paranormal phenomena verging on the incredible, they might not have suffered from hallucination or illusion induced by seance-room conditions. In *Proc.* xxi Count Solovovo and Alice Johnson had an interesting and detailed discussion of this problem, to which he returned in his paper 'Les Phénomènes Physiques du Spiritisme' (*Proc.*, xxv, 413-46). These papers show

well the strong but reasoned spirit of criticism which informed all his work.

He was for many years the Society's Hon. Secretary for Russia. At the time of the Revolution he was serving on a diplomatic mission abroad, and so escaped with his life though with the loss of his considerable wealth. He faced with great courage the change in his fortunes, and earned a modest living by writing for the press in Brussels and elsewhere, being well qualified for this by his mastery of several languages and his experience of public affairs. For several years he reviewed for our *Journal* continental journals devoted to psychical research.

On rare occasions the Society has recognized eminent service to psychical research by conferring Honorary membership, and in 1918 Count Solovovo was clearly marked out for this recognition. In 1936 he made his home in London, and gave our members the pleasure, which I think he shared himself, of a personal interchange of opinions on subjects which all his life long had engaged his interest and activity. Some years before his death he further strengthened his ties with us by acquiring British citizenship.

W. H. S.



